in two fhort lines; my fidelity to the king not per-
mitting me to fend you any more.

N·A LTFRI∨S·D∨LC
DFⅯ·C∨RI S·CR∨DF

This is the fize and fhape of the characters. In
this bit there are eight lines. There are other bits
with many other words; which are all preferved in
order for their publication.

There have been found likewife very lately three
beautiful ftatues of marble, and one of them excellent:
Six heads of bronze, of which there is one, that
gives hopes of finding the ftatue it belongs to. It is
a young Hercules, of a kind of work, that has no fellow
in the way of metal, having the hair finifhed in a
furprifing manner. Likewife feveral little figures of
metal; a fiftrum very neat and well preferved; and
there is not a day paffes, in which they do not bring
to me fome curiofities newly found.

X *A Tranflation and Explanation of fome
Articles of the Book intitled,* Theorie de
la Figure de la Terre; *by Monf.* Clairaut,
of the Royal Academy *of* Sciences *at* Paris,
and F. R. S.

Read Feb. 15,
1753.

MR. Short, in his account of Father
Frifius's *Difquifitio mathematica
in caufam phyficam figuræ et magnitudinis telluris
noftræ,* having reported that philofopher's fentiments

K   on

on my reflections upon the same matter, without taking the trouble to examine whether they were founded upon the truth or not, I find myself under the necessity of laying before the Royal Society the passages of my book, which, having been misunderstood by F. Frisius, have occasioned the misconstruction, which he has made of my sentiments, either upon the trust I give to the actual operation made for discovering the figure of the earth, or Sir Isaac Newton's theoretical inquiries about the same subject.

The expressions of Father Frisius, referr'd to by Mr. Short, are as follow:

" Quia tamen plerique omnes hucusque, aut nihil
" pro figura telluris determinanda ex iis observationi-
" bus deduci posse cum geometra celeberrimo Rug-
" gero Boscovik autumârunt, aut exinde cum Ill.
" Clairaut, Bouguer, aliisque, contra incomparabilem
" virum ac prope divinum Isaacum Newton insur-
" gentes, admirabilem ipsius theoriam facto minus
" respondentem dixerunt, assignatamque in prop. 19.
" lib. 3. *Princip. Mathem.* terrestrium axium pro-
" portionem à vera absonam omnino esse, alios mihi
" observationibus parum, alios nimis tribuere visum
" est, omnes ferme oppositis erroribus peccâsse, ubi
" res neque aurificis lance, neque molitoris, ut aiunt,
" statera librandæ sunt."

This, when compared with the propositions of my theory, which they are relative to, will appear, I hope, quite incoherent: and I cannot shew it more clearly, than by translating the last chapter of my book, to which F. Frisius refers the reader.

For

For the better underſtanding of that chapter, it is proper to know, that the chief reſults of the precedent inquiries are theſe theorems:

1. Suppoſing the earth in its former ſtate compoſed of ſeveral fluids of different denſities, and ſettled all in equilibrium by the laws of gravity and centrifugal forces, the ſurfaces ſeparating the different mediums will always affect the form of a curve; which is ſo near to the ellipſis, that it may be ſuppoſed ſo, without any error of the leaſt moment.

2. That, in the caſe of the denſer fluids being nearer to the center, as hydroſtatics require, the ſpheroid will always be leſs flat than in the homogeneous one, and *vice verſa.*

3. And as to the diminution of the gravity from the pole to the equator, it will always follow the oppoſite rule; *viz.* if the ſpheroid be denſer towards the center, the gravity will decreaſe in a leſs ratio than in the homogeneous ſpheroid, and *vice verſa.*

4. That if δ repreſent the fraction found out for the difference of diameters, $\frac{1}{115} - \delta$ will expreſs the total diminution of the gravity from the pole to the equator, not only in the caſe of the ſpheroid ſuppoſed originally fluid, but in any ſuppoſition of variation for the denſities and proportion of the diameters of the beds, provided they be elliptical.

Theſe premiſed, let us proceed to the ſaid laſt chapter of the theory of the earth's figure; in which the principles laid down in the preceding chapters are compared with the obſervations.

§ LXVIII. *For the diminution of the gravity from the North to the South.*

It has been feen in the preceding chapter, that when a fpheroid is not fuppofed homogeneous, the diminution of the gravity from the pole to the equator will be greater than in the cafe of homogeneity. Hence, if my theory holds in our globe, the whole decreafing of the gravity will be equal to $\frac{1}{230}$ or greater, and never lefs; fince the ratio of 230 to 231 will (§ XXI.) exprefs the ratio of the action of gravity at the equator and pole, when the fpheroid is homogeneous.

And this conclufion of my theory quite agrees with experience; for, from all the obfervations relating to the gravity made in feveral places of the globe, either by actual meafures of the fecond pendulum, or by the difference of duration of the fame pendulum's vibrations, it appears, that the gravity decreafes from the north to the fouth in a greater ratio, than it would be if the total diminution from the pole to the equator were only $\frac{1}{231}$.

§ 69. *For the proportion of the two diameters.*

Suppofing, as in the precedent chapter, the earth originally fluid, it follows, from the § LXV. that the ratio of the two diameters cannot exceed that of 230 to 231; fince, § XX. 230 to 231 is the ratio in the cafe of the homogeneous fpheroid; and as the menfurations of the gravity cannot agree with the fuppofition of the homogeneity, the diameters of the earth ought to be in a ratio lefs than 230 to 231.

Without

Without adhering to the fuppofition of the earth's
being formally fluid, but admitting, as in the chap.
3 and 4, all generality poffible in the variation of
denfity and ratio of diameters of the beds or ftrata
laid down from the center to the furface, there will
ftill happen a difference of the diameters lefs than $\frac{1}{130}$.
For, by § L. the total diminution of the gravity
from the pole to the equator being fubtracted from
$\frac{1}{115}$, the remainder is the difference between the dia-
meters. Now the diminution of the gravity having
been found greater than $\frac{1}{230}$, the ellipticity or dif-
ference of diameters ought to be lefs than that frac-
tion, and confequently the ratio of diameters lefs than
230 to 231.

That confequence of my theory is not fo happy as
the preceding; for the degree meafured in the north
compared to that of France give the two diameters as
177 to 178, which ratio is greater than 230 to 231,
inftead of being lefs, as the theory would require.

As the meafures made in the north have been per-
formed with great care and exactnefs, their refult
feems at firft to be preferred to that of my theory.
But a reflection upon the errors unavoidable in actual
meafures, and an examination of the limits of thefe
errors, will fhew, that, without violating the mea-
fures, they would be brought nearer the theory, and
even agree with it.

For, by a convenient calculation, it will be found,
that a diminution lefs than 60 toifes, made to the dif-
ference between the degrees of Paris and Tornea,
would give the diameters in the ratio of 230 to 231.
And if it be confidered, what is the fmallnefs of an er-
ror of 60 toifes, when divided in two operations, which
<div align="right">require</div>

7

require fo great a number of aftronomical and geo-
graphical obfervations, it will be thought, that an
error a little larger may be fuppofed, without difpa-
raging either our operation, or Mr. Picard's; and thus
theory and experience would agree.

Suppofing, for example, that the difference be-
tween the degrees oɩ Paris and Tornea has been found
too great by 80 toifes, the difference between the
two diameters will come out of about $\frac{1}{260}$, which,
fubtracted from $\frac{1}{115}$ gives $\frac{1}{206}$ for the diminution of
the gravity from the pole to the equator. And fuch
a conclufion would agree pretty well with the obfer-
vations made in France and Lapland with the excel-
lent clock of Mr. Graham.

However, altho' the errors to be fuppofed in the
operations, to reconcile them with my theory, be in
themfelves fmall enough, I fhall abftain from affert-
ing, that they have been committed. It is a fact
not to be decided, till after the refult of the ob-
fervations, which are expected from Peru. For
the great difference, which is to be found between
the degrees of Quito and Tornea, is the only means of
knowing, whether the diameters be lefs or greater
than 230 to 231.

Were the queftion only to demonftrate the flatnefs
of the earth, the meafures of the degree of Paris and
Tornea would be full fufficient; but to difcover the
true ratio of diameters, is what can be performed only
by the comparifon between the degrees, whofe mutual
diftance is the greateft.

Such a ratio once fixed, if it happen to be lefs than
230 to 231, it will be very eafy, by the preceding
theory, to imagine fome hypothefis for the infide of
the

the earth, which fhall agree with both theory and obfervation, whether admitting the fuppofition of the original fluidity of the globe, or not.

But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231, I own, that not only the theory eftablifhed in this fecond part of my book muft be abandoned, but it would be very difficult to reconcile the meafures of the pendulums with thofe of the degrees in Sir Ifaac's fyftem. And I dare, fay, that the fuccefs in that cafe would hardly depend upon any natural hypothefis.

The fubfequent LXX article containing only a proof, that the preceding theory agrees with any ratio between $\frac{1}{23}$ and $\frac{1}{9}$, for the quantity, which expreffes the excefs of Jupiter's equator above its axis, there is no neceffity for the tranflation of the arguments leading to a refult fo anfwering to the obfervations; and I pafs to the conclufion of that article, which ends my book.

The preceding theory agreeing with all the meafures of the pendulum, and obfervations of Jupiter's diameters, if, befides, it happen, that the meafures expected from Peru give, when compared with thofe of Lapland, a difference of diameters lefs than $\frac{1}{130}$, this theory will have all poffible confirmation, and the univerfal gravitation fo well agreeing with the motions of the planets will alfo agree with their figures.

Now I beg every candid reader to examine, whether, in that chapter quoted by F. Friffus, I have too much relied upon the certainty of obfervations, and attempted to difparage Sir Ifaac Newton's difcoveries.

In

In the firſt place, I will aſk of Father Friſius, if, before the operations, which I depended upon, were performed, I could eſtabliſh any thing againſt their agreeing, or not, with Sir Iſaac's propoſition about the ſame matter?

He perhaps will anſwer, That my remark of the LXIX art. *But if the diameters were found undoubtedly in a greater ratio to one another than* 230 *to* 231, imports, that I was not thoroughly convinced, that what care ſoever would be taken by the gentlemen ſent into Peru, they never would be able to meaſure their degree with a ſufficient exactneſs, to conclude, from its length, compared with that of the other degrees, whether the diameters were in a greater or leſs ratio than 230 to 231; and conſequently he will think, that my being in ſuſpenſe about it was an offence againſt Sir Iſaac's theoretical determination. Then, I requeſt Father Friſius to tell me, why he is ſo good as to commend operations ſo void of uſe as thoſe, which tended only to diſcover what was demonſtrated before, and needed not to be confirmed, ſince it could not be invalidated.

Perhaps Father Friſius, in repreſenting me as depending too much upon the obſervations, relied on theſe expreſſions of the LXIX art. *As the meaſures of the gravity cannot agree with the ſuppoſition of the homogeneity :* and I confeſs, that it ſeems to me impoſſible to reconcile the great number of all the meaſures of that ſort with the table, which follows the homogeneity. For the ſimplicity of the means made uſe of in the performance of thoſe meaſures cannot admit the errors, which ſhould be ſuppoſed to bring them to Sir Iſaac's theory: but as this theory is founded

on

on the homogeneity, which is only a mere fuppo-
fition ; and as he has himfelf fufpected, in his fecond
and third edition, that the internal parts of the earth
might be denfer than thofe towards the fuperficies, I
do not fee how I oppofe myfelf that illuftrious philo-
fopher, when I affume the fame hypothefis, as he
does. As I fhall ufe all poffible endeavour to under-
ftand F. Frifius's meaning, I hazard this conjecture.
Seeing that I thought favourably enough of the exact-
nefs to be obtained in aftronomy, when obfervations
have been already made in great numbers, and with
all poffible care, to fuppofe them fit to let us know,
whether the diameters are in a greater or a lefs ratio
than 230 to 231 ; and being informed afterwards,
that the operation made in Peru led thofe, who have
made ufe of it, to imagine the fpheroid flatter than the
homogeneous, he concludes, that I cannot help think-
ing like them, and accordingly indulges himfelf in ex-
pofing, how much I over-rate the validity of obferva-
tions, and how little I know the fubmiffion due to a
propofition of Sir Ifaac ; which, I muft fay, by the
bye, that great man has never himfelf given as im-
poffible to be oppofed by experience. But yet I would
afk of F. Frifius, wherefore he will guefs at my fen-
timents, whilft I have not given room to know them
on that point ? How can he know, whether, fince
the examination of all the meafures, I have not found
any way to reconcile them with the theory ? Which
I fay in no manner as a hint I intend to make any
corrections in thofe meafures, but merely to fhew the
little foundation, which F. Frifius had to reprefent me
as he has done.

I.   However

However difficult it may be to account for Father Frifius's expreffions, I fhall hazard yet another conjecture. His great zeal for Sir Ifaac, for which he is certainly to be commended (if not blinded by that zeal) has hindered him from diftinguifhing between the different ways of oppofing that great man's fentiments. Perceiving then, that my calculations (§ L. Part II.) had led me to a refult quite different from Sir Ifaac's affertion, (Prop. XX. lib. 3.) he was offended at my boldnefs to fuch a degree, that he was unable to examine impartially what I faid; and, inftead of difcuffing a mathematical queftion quite independent of any actual meafure, wherein if I were miftaken, he would have forced eveiy geometrician to condemn me, he has fuppofed, that I have built my argument upon an operation, which was not performed at the time when I wrote.

This conjecture would appear to me the true caufe of F. Frifius's error, if it were not inconfiftent with a proceeding of his towards Sir Ifaac, which I will venture to relate. After F. Frifius has examined himfelf the 19 problem of the third book of the *Principia*, which is much lefs complicated than that I fpoke of, the truth of which is inconteftable, he finds, by his own miftake, a difagreement with the refult of that propofition, and charges that illuftrious author, without the leaft apology, with an error, which, fays he, (quite from the purpofe) is the fixth, that has been found in the fame work, and alfo gives an enumeration of the five others, altho' they are not at all concerned in the queftion.

I cannot forbear faying, that the manner, in which I have propofed my remarks upon the 20th propofition

of

of Sir Ifaac, has nothing of that flight way of treating
fo great a man; and as my utmoft wifh is to be judged
on that account by the Royal Society, I fhall relate
what were my objections; which I cannot effect in
a more concife and clear method, than by giving the
tranflation of the article, which contains it.

§ LI. of the fecond part of the theory, &c.
" *In which is feen what had induced Sir* If. Newton
" *to think, that the planets, when denfer at the cen-*
" *ter than at the furface, ought to be flatter than in*
" *cafe of homogeneity.*

" Some years ago I gave, in the *Philof. Tranf.*
" N° 449, the theorem of the precedent article;
" and on this occafion I mentioned a paffage of Sir
" Ifaac contrary to it. Not having at that time
" looked into the fecond edition of his *Principia*, I
" could not know what had engaged that illuftrious
" philofopher to think fo; and far from fufpecting
" any miftake in his propofition, I was contented to
" think, that the difference between our conclufions
" arofe from a different way of conceiving the infide
" of the earth; and I imagined, that he had happen'd
" to fall upon fuch a difpofition of parts, as would
" anfwer to his affertion. I then follow'd only his
" commentators, and efpecially Dr. Gregory, fhew-
" ing, that his explanation of Sir Ifaac's conclufion
" was wrong, as grounded upon a propofition, which
" did not hold in the prefent cafe For that pro-
" pofition (which is, that the gravity at any point
" of the earth is inverfedly as the diftance from the
" center) has only room, when the earth is homo-
" geneous; and, confequently, ought not to be made

" ufe

" ufe of, when the denfity is greater towards the
" center than at the fuperficies.

" Since I have difcover'd, that the theorem, the
" demonftration of which I had given in the *Philof.*
" *Tranf.* for the cafe of beds fuppofed of the fame
" ellipticity, has room in an infinity of other fuppo-
" fitions, I have taken greater care to difcover what
" could have induced Sir Ifaac to think, that the
" earth is flatter, as the gravity is more decreafing
" from the pole towards the equator; and I believe
" I have found it out in the fecond edition of the
" *Principia,* and it is, for having built upon the
" fame argument as Dr. Gregory.

" In p. 386, after having obferved, that the ex-
" periments gave a diminution of two lines to the
" fecond pendulum from the pole to the equator, he
" argues thus: Since, fays he, the cafe of homoge-
" neity afforded only $1 \frac{87}{1000} 4$, the difference $7\frac{1}{6}$ miles
" between the two diameters (which follow'd from
" the fame hypothefis) is to be magnified in the ratio
" of $1 \frac{87}{1000}$ to 2, and it will come out $31 \frac{7}{12}$ miles
" for the real difference. For, purfues he, the re-
" tardation of the pendulum at the equator denotes
" the diminution of the gravity in that place; and the
" lighter the matter is there, the higher will it rife
" to be equiponderant with that of the pole.

" Further, *p.* 387, examining the meafures of the
" degrees of latitude made in France by M. Caffini,
" by which the earth is higher at the pole than
" at the equator by about 95 miles, he pretends,
" that accordingly the pendulum fhould be longer at
" the equator than at the pole by about half an inch.
" And all that, methinks, fhews the opinion, which

" Sir

" Sir Ifaac was of, that, in any cafe whatfoever, the
" equilibrium requires a gravity inverfedly propor-
" tional to the length of the columns, which pro-
" portion, as I hope to have evinced, is only demon-
" ftrated in the cafe of homogeneity, and is not true
" in general. Thus, what I argued in the *Phil.*
" *Tranf.* againft Dr. Gregory, has alfo room againft
" Sir Ifaac."

By all that I have faid, every body may judge, whe-
ther differing from Sir Ifaac's fentiments on a point,
which I had for fo long a time examined, I did not ex-
prefs my difagreement with him in as decent a manner,
as any one fhould, when fpeaking of fo great a man.
And in cafe the Royal Society thought fome altera-
tions were to be made in the form of my remarks, I
declare, that I fhall execute it, as may be prefcribed
to me by that illuftrious company. But I cannot
help thinking, that, unlefs thofe, who would exa-
mine my demonftrations, find fome error in them,
no alteration is requifite to be made in my expref-
fions. I defire then either F. Frifius, or any geo-
metrician, who thinks the queftion worth his exa-
mination, to take the trouble of reviewing my calcu-
lations, and to believe me ready to acknowlege my
error, when fhewn to me by a candid and impartial
examiner.

XI.